SATURDAY, AUGUST 28, 1880.

BOOKS RECEIVED.

A TREATISE ON COMPARATIVE EMBRYOLOGY. By Francis M. Balfour, M. A., F. R. S., in two volumes. Vol. 1. Macmillan & Co., London. 1880.

Macmillan & Co. have recently forwarded to us this very interesting volume of over 300 pages, abundantly illustrated, the first part of a work upon which we believe the author has been constantly occupied since the publication of his "Development of the Elasmobranch Fishes," in 1878. The second volume is still in press, and will deal with the Embryology of the Vertebrata; while this, the first volume, is devoted to the Invertebrata, omitting the Protozoan forms, and it includes in the beginning of the book an outlined history of the Ovum, as it appears in both the Vertebrate and Invertebrate types.

This is by far the most important book that Mr. Balfour has undertaken, and, in practical importance, takes the precedence of any work that has yet appeared in this branch of biological science.

In his "Elements of Embryology," written with Prof. Foster of Cambridge in 1874, and his "Elasmobranch Fishes," besides numerous contributions to the *Quarterly Journal of Microscopical Science*, and the societies, the author has won a position among European embryologists which makes this work doubly valuable. Merely for an expression of his opinion on many mooted questions, the book will be welcomed on both sides of the Atlantic; but it contains, moreover, as full a history of every form as scientific investigation up to the present time has furnished, with a manifest endeavor throughout to do justice to every investigator. The absence of such a work has long been felt by American students. Few of our libraries have been able to obtain, to anything like completeness, the works of biological specialists; in great measure because such works appear in the form of scattered memoirs, difficult to procure even at the time of publication. In the full field of French, German, Italian and Russian investigations, the danger of completely overlooking the researches of others is constantly discouraging. It may be in appreciation of this general want that the author has placed full notices of his sources of information at the end of each chapter where such reference is made, and, in addition to an index of subjects, has inserted at the close of the volume a classified bibliographical index. This renders each subject exceptionally clear, and places the student in a much fairer way of hunting up the literature of his specialty than has been possible hitherto. In these respects, the book is a model for works of this character.

The science of embryology, now ripe for an eclectic work of this description, has grown rapidly from its infancy in the middle of the present century to the importance of a separate and elaborate branch. With its voluminous literature, it is strange that with one exception, a small volume by Packard, no attempt has been made to collate opinions or handle the subject as a whole. In the phylogenetic light alone, Embryology ranks as a vital portion of Biology; in this connection may be quoted a few lines from the introduction: "It has long been recognized that the larvae and embryos of each group pass, in the course of their development, through a series of stages in which they more or less completely resemble the lower forms of the group." The author shows the bearing of the Darwinian theory upon this fact. While morphology may establish the relations of genera, we turn to Embryology for the basis of a wider classification. Its bearing upon Comparative Anatomy is a patent fact. So it is in the interest of the history of development, or in the relation of a given type to its progenitors, as well as in the morphology and physiology of individuals that embryology is of constantly increasing importance. This is, in part, pointed out by the author in the introduction of the work. More specifically he states the aims of the present work as two-fold: (1) To form a basis for Phylogeny (or the history of the race or group); and (2) to form a basis for Organogeny (or the origin and evolution of organs).

In course of a review of the phenomena of reproduction, as witnessed among the Protozoa and Metazoa, the transition from single to compound organisms is clearly stated: "It must be remembered that a single individual Metazoon, is equivalent to a number of Protozoa coalesced to form a single organism in a higher state of aggregation. It results from this that the segmentation of the ovum which follows the sexual act may be compared to the product of conjugation breaking up into spores, the difference between the two processes consisting in the fact that in the one case the spores separate to form an independent organism, while in the other they remain united, and give rise to a single compound organism."

The ovum is treated of in the first chapter as the natural point of departure in the cycle of development—first in its general, then in its special histories in different types. In the second chapter upon Impregnation and Maturation, an account is given of the remarkable researches of Fol and Hertwig, which surpass in the minute history of these changes the observations of any other naturalists. A chapter on Segmentation closes this introductory portion of the work. In view of the fact that this phenomenon hinges upon the disposition of, the presence, or the absence of food-yolk, the author proposes terms for three corresponding types of ova, as follows: (1) Alecithal for ova without food-yolk, or where it is evenly distributed; (2) Telolecithal, where the yolk is concentrated at one pole; (3) Centrolecithal, where the yolk is concentrated in the centre.

The reader is now ready for Part I, Systematic Embryology, in which the history of each group is treated from the formation of the germinal layers onwards, beginning with the simple parasitic forms, the Dicyemidae and Orthonectidae, passing through each invertebrate family whose development has been studied, and closing with the Echinodermata and Eteropneusta.

A detailed review, even of the author's conclusions, would be obviously out of place. Attention may, however, be called to one or two passages of interest, not only to the specialist, but to the general student of biology. The Coelenterata form an attractive group from the fact that they rarely, if ever, pass from the two layered condition, and the lowest forms, even when adult, "do not rise in complexity much beyond a typical gastrula." The larval form, the planula, is common to all except the Ctenophora. Referring to this, the author remarks: "Paradoxical as it may seem, it appears to me not impossible that the Coelenterata may have had an ancestor in which a digestive tract was physiologically replaced by a solid mass of amoeboid cells."

The chapter on the development of the Mollusca is very full and interesting.

In summary of the group Arthropoda, the genealogy of the Tracheata and Crustacea tends to throw doubts upon the uniting of the whole of the arthropoda into one phylum. In the first place, the Tracheata are descended from some terrestrial annelidan type allied to *Peripatus*. [This is the interesting proto-tracheate form collected by Mr. Moseley on the Challenger expedition, and found by him to possess trachea and nephridia, two organs which respectively demonstrate its affinities in opposite lines to the tracheate and annelidan groups.] The Crustacea on the other hand are clearly developed from a phyllopod-like ancestor, which can in no way be related to *Peripatus*. The conclusion that the Crustacea and Tracheata belong to two distinct phyla, is moreover confirmed by their development.

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COAL.

BY P. W. SHEAFER, M. E., POTTSVILLE, PA.

II.

The fearful loss of good material involved in mining and preparing Anthracite, as shown in the accompanying tables, though greatly to be deplored, seems to be almost inevitable. The disposition of the coal in large solid beds, and in highly inclined positions, involves strong supports to keep the superincumbent mass from crushing and closing the avenues to the mines; and these supports must consist of massive pillars of the solid coal itself. Wooden props, however ponderous and strong, can only be used for the minor supports. Some of this pillar coal is ultimately removed, but much of it is inevitably lost, especially in the larger beds which frequently range from 20 to 40 feet in thickness, and are often inclined at an angle of from 40 to 70 degrees.

It is estimated that not more than 66 per cent. of the coal is ever taken out from the mines. That which is brought to the surface is run through a huge structure from 80 to 100 feet high, very appropriately called a "breaker," ingeniously contrived for the destruction of coal. There are over 300 of these immense buildings in the Anthracite region, costing on an average $50,000 each, or an aggregate of $15,000,000. To the top of these the coal is hoisted, and then descends through a succession of rolls and screens, emerging at the bottom, in a series of assorted sizes, from huge blocks of lump coal to unmerchantable dust, which forms a grievously large proportion of the whole. This process involves a loss of good coal, equal to 20 or 25 per cent. of the entire quantity mined. For the coal wasted in mining, say 40 per cent., and in preparing, 25 per cent., no one is paid; it is a total loss to landowner, miner and shipper.

Plans for utilizing the waste coal dirt, or culm of Anthracite collieries, have been frequently suggested, but none have come into general use. The Anthracite Fuel Company, at Port Ewen, on the Hudson, in 1877, used 90 per cent. coal dust and 10 per cent. fuel pitch, and made 300 tons of fuel per day, consuming over 50,000 tons of culm. The Delaware and Hudson Company also use at their mines 60,000 tons per annum. They now ship all their coal down to pea sizes, and consume the culm in generating steam. If all our coal companies would follow this excellent example it would enable them to sell half a million tons more coal, and burn the same amount of refuse, thus earning or saving half a million dollars per annum, to add to their revenues. The Philadelphia and Reading Railroad Company has recently introduced a method of burning coal dust in the furnaces of its engines, and the plan appears to meet with success.

The amount of water which drains into a mine from a mile or more of surface is enormous, for the average amount of rain and snow fall is 58,840 cubic inches per square yard annually, and the mines are liable to absorb not only the rain fall on the surface immediately over them, but all that which by contour of the surface, or by converging strata, tends towards them. On an average possibly five tons of water are hoisted for every ton of coal raised—another loss chargeable to mining.

The preponderance of waste coal seems excessive; but the writer's experience in surveys of certain tracts of land, and in preparing maps which show the area exhausted, compared with the amount marketed from ten or more collieries, in a period of 20 years, proves that the loss is not over-estimated, especially in the Mammoth Bed, whose average thickness is 25 feet. An eight-foot bed of coal yields much better in proportion. When they exceed six or eight feet in thickness, especially if steeply inclined, they are not only expensive to mine, but a large proportion of the coal must be left to support the rocky roof.

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The total amount of coal still to be mined, according to the accompanying tables, is 26,361,076,000 tons. The total waste, as experience has shown, is equal to two-thirds of the coal deposit, and reaches the appalling amount of 17,574,050,666 tons, leaving us only 8,787,075,533 tons to send to market. In all our calculations of Anthracite we have counted the area as if in a level plain, and made no allowance for the undulations which must necessarily increase the amount of coal. But as many of the flexures are abrupt and broken, making much faulty and refuse coal, it will cover any over-estimate of area or thickness we have made in our calculations.

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sent to market in the 58 years from 1820 to 1878, inclusive. Our consumption now amounts to 20,000,000 tons annually. The increase of production for the past ten years has been 187,112,857 tons. At this rate we shall reach our probable maximum out-put of 50,000,000 tons in year 1900, and will finally exhaust the supply in 186 years.

The present product of the Anthracite coal fields is (1878) as follows:

Southern	50	Collieries	6,282,226 tons.
Middle	161	"	3,237,449 "
Northern	132	"	8,085,587 "
Total	343	"	17,605,262 "

At this rate the eastern end of the northern field is being rapidly exhausted. The middle field, too, which contains the lower productive coals, is likely to cease extensive mining about the year 1900; while the western portion of the northern field, extending from Pittston to the western end, and the southern field from Tamaqua to Tremont, comprising about 100 square miles, which contain more coal beds and deeper basins, must furnish the supply for the coming years.

Partially successful experiments have been made to use petroleum as a substitute for coal to some extent. But is it not already evident, under the reckless prodigality of production, that this occult and mysterious supply of light and heat and color will be exhausted before the Anthracite, and can, at best, only temporarily retard the consumption of the latter?

As already intimated, the question of the exhaustion of our coal supply is scarcely more at the present time than a curious and interesting calculation. It has not yet become so grave and portentous as in Great Britain, where a commission, with the Duke of Argyle, Sir Roderick Murchison and Sir W. G. Armstrong at its head, was recently appointed by Parliament to ascertain the probable duration of the coal supplies of the kingdom. There it is serious indeed; for when Britain's coal fields are exhausted, her inherent vitality is gone, and her world-wide supremacy is on the wane. When her coal mines are abandoned as unproductive, her other industries will shrink to a minimum, and her people become familiar with the sight of idle mills, silent factories and deserted iron works, as cold and spectral as the ruined castles that remain from feudal times.

The modern growth and ultimate decadence of this great empire may be calculated from the statistics of her coal mines. In 1800 her coal product was about 10,000,000 tons; in 1854 it was 64,661,401 tons; and in 1877 it swelled to 136,179,968 tons. This period was a time of continued prosperity, when England ruled the world financially and commercially. In the 23 years from 1854 to 1876, inclusive, she produced the enormous quantity of 2,210,710,091 tons of coal; and, more wonderful still, exported only 222,196,109 tons—say ten per cent—consuming the rest within her own borders.

The average increase of her annual output has been 3½ per cent. Will it so continue? Or has she reached the summit of her industrial greatness and commercial supremacy, and will they now decline, and with it her naval and military power, the subservient agent, and, to a large extent, the creature and result of those great interests?

Our Anthracite product, compared with the coal product of Great Britain, is so small as to really seem insignificant. The English Commission counts as available all coal beds over one foot thick—we count nothing under two and a half feet thick, nor below 4,000 feet in depth—showing a net amount in the explored coal fields of 90,207,285,398 tons; estimated amount in concealed areas, 56,273,000,000 tons; total, 146,480,285,398 tons, distributed as follows:

	Explored.	Unexplored.	Total.
England	45,746,930,555	56,246,000,000	101,992,930,555
Wales	34,461,208,913	....	34,461,208,913
Scotland	9,843,465,930	No estimate.	9,843,465,930
Ireland	155,680,000	27,000,000	182,680,000
Total	90,207,285,398	56,273,000,000	146,480,285,398

The exhaustion of this magnificent mass of coal at this present rate of increase, viz.: three and a half per cent. per annum, is estimated by Professor Jevons as follows:

1876, actual output	133,300,000 tons.
1886, estimated annual output	186,600,000 "
1896, " " "	261,200,000 "
1906, " " "	365,700,000 "
1916, " " "	512,000,000 "
1926, " " "	716,800,000 "
1936, " " "	1,003,500,000 "

Thus in sixty years the output would be nearly eight times the present amount, and about one-fourth of the total amount to be found in Great Britain.

This vast estimate seems too enormous. It does not allow for great loss when cost of labor and much competion will prevent the working of small coal beds under two feet in thickness, or for the cost of mining when from 2000 to 3000 feet deep. Nor is it possible that Great Britain's industries and export trade combined will ever require so great a quantity. Modern discoveries and improvements, in applied science, tend to diminish the consumption. The 8,000,000 tons annually required for gas-works may be materially reduced by the use of the electric light. The domestic consumption, now equal to one-fourth the product, or 33,000,000 tons a year, may increase. But will not the iron manufactures be on the wane, and her coal exports—now ten per cent. of her coal product—fall off as those of other countries increase?

We have about 340 collieries and produce 20,000,000 tons per annum, or about 60,000 tons each. Great Britain has nearly 4000 collieries, and mines 132,000,000 tons, or 33,000 tons per colliery. The greater the yield per colliery the less the expense in mining. If we decrease the number of mines and increase their capacity not only to raise the coal, but to exhaust a constant current of foul air and dangerous gases, clouds of powder smoke and millions of gallons of water, we will reduce the cost of mining. Most of the Anthracite mining in the United States is now done at a less depth than 500 feet vertical; but as the coal nearer the surface becomes exhausted, the mines must go deeper and become more expensive.

What a folly it is to boast of our world's supply of Anthracite, and feverishly endeavor to force it into foreign markets, when we can so readily foresee its end? Would it not be wiser to limit its product, restrict its sale to remunerative prices, and consume it at our own firesides, and in our own manufactures?

The monopoly of the Anthracite coal fields by some seven corporations, which, according to the accompanying tables, now control about two-thirds of the whole, and the best coal area, must prove, under economic management, a profitable investment for their stockholders. Mining, selling and transporting their own coal, as they do, individual enterprise cannot hope to compete with them, and must vanish from the ground, and their only rivalry will be with each other, and with the Bituminous trade. Fortunately for the public, this rivalry will always be keen enough to keep the price of coal at a fair low rate of cost and profit.

The coal resources of Great Britain are all developed now, and in process of depletion; while in this country when our 470 square miles of Anthracite are exhausted, we have more than 400 times that area, or 200,000 square miles of Bituminous, from which to supply ourselves and the rest of mankind with fuel. The coal product of the world is about 300,000,000 tons annually. The North American continent could supply it all for 200 years. With an annual production of 50,000,000 tons, it would require twelve centuries to exhaust the supply. But with a uniform product of 100,000,000 tons per annum, the end of the Bituminous supply would be reached in 800 years. What the annual consumption will be when this continent supports a teeming population of 400,000,000 souls, as will be the case some day, must be left to conjecture. But with half that population, as energetic, restless, and inventive as our people in this stimulating climate have always been, under the hopes of success, such a country as this constantly holds out much to tempt ambition and reward enterprise.

If it be true, as Baron Liebig asserts, that civilization is the economy of power, we have it in our immense areas of Bituminous coal. There is no known agent that can answer as a substitute for the vast power and almost limitless usefulness of coal in its general adaptation to the wants of man; and that nation will maintain the foremost rank in enlightened modern civilization which controls, to the fullest extent, while it lasts, this wonderful combination of light and heat and force. We are wiser than our fathers; and from the modest but sublime altitude to which we are lifted by physicial science, and the far extended range of mental vision which it opens up to us, we can see farther into the plans of Providence than those who went before us, and can conjecture the early, if not the remote, future of the human race in our land and in other lands.

Happy that people whose legislators study the best mode of developing the natural resources of their country, and whose great men become great by improving the condition and promoting the welfare of the human race. The greatest of England's five Georges was not either of those who wore the crown, but plain George Stephenson, of Manchester, who rolled the world farther along the path of progress than all the others; and none of the royal Jameses did half so much for the civilization of his country as James Watt, whose boyish study of the steaming tea-kettle developed the giant power that does the world's work with an energy that is tireless and irresistible.

ETHNOLOGY.*

FRAGMENTARY NOTES ON THE ESKIMO OF CUMBERLAND SOUND.

By Ludwig Kumlien.

II.

They have an interesting custom or superstition, namely, the killing of the *evil spirit* of the deer; some time during the Winter or early in Spring, at any rate before they can go deer-hunting, they congregate together and dispose of this imaginary evil. The chief *ancoot*, *angekok*, or medicine-man, is the main performer. He goes through a number of gyrations and contortions, constantly hallooing and calling, till suddenly the imaginary deer is among them. Now begins a lively time. Every one is screaming, running, jumping, spearing, and stabbing at the imaginary deer, till one would think a whole mad-house was let loose. Often this deer proves very agile, and must be hard to kill, for I have known them to keep this performance up for days; in fact, till they were completely exhausted.

During one of these performances an old man speared the deer, another knocked out an eye, a third stabbed him, and so on till he was dead. Those who are able or fortunate enough to inflict some injury on this bad deer, especially he who inflicts the death-blow, is considered extremely lucky, as he will have no difficulty in procuring as many deer as he wants, for there is no longer an evil spirit to turn his bullets or arrows from their course.

They seldom kill a deer after the regular hunting season is over, till this performance has been gone through with, even though a very good opportunity presents itself.

Salmo salar, and one other species of *Salmo* that I could not procure enough of to identify, are caught to some extent in June and September in some of the larger fjords; they are mostly caught with a spear, but sometimes with a hook. (For description *vide* under hunting-gear, etc.)

When these fish are caught, they are put into a seal-skin bag, and it remains tied up till the whole becomes a mass of putrid and fermenting fish, about as repulsive to taste, sight and smell as can be imagined. *Cottus scorpius*, which contributes so largely towards the Greenlander's larder, is not utilized by the Cumberland Eskimo, except in cases of a scarcity of other food supplies; the fish is abundant in their waters, however, and fully as good eating as they are on the Greenland coast.

Birds and their eggs also contribute towards their sustenance in season; they are extremely fond of eggs, and devour them in astonishing quantities.

The "black skin" of the whale, called by them *muktuk*, is esteemed the greatest delicacy. When they first procure a supply of this food, they almost invariably eat themselves sick, especially the children. We found this black skin not unpleasant tasting when boiled and then pickled in strong vinegar and eaten cold; but the first attempts at masticating it will remind one of chewing India rubber. When eaten to excess, especially when raw, it acts as a powerful laxative. It is generally eaten with about half an inch of blubber adhering.

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In Summer, especially when on hunting excursions, they very often "fry" meat by making a little fireplace of stones, and laying a flat piece of stone on the top. The opening to receive the fuel supply is to windward. For fuel at such times they use *Cassiope tetragona* and *Ledum palustre;* these shrubs make a quick and very hot fire. It would be comparatively an easy task for these people to gather enough *Cassiope tetragona* during the Summer to burn during the coldest weather, and not rely wholly upon blubber.

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I found among some of these people a little spoon, or rather a miniature scoop, made of ivory, which they used to drink the soup with; it appears to be an old utensil, now going fast out of use, for they can now procure tin mugs. A reindeer's rib, pointed at one end, is used to fish up the meat with, and sometimes to convey it to the mouth. These instruments are found in the graves, but seem to be little used at the present day.

When a seal is brought to the encampment, especially if they have not been plenty for some days, all the villagers are invited to the hut of the lucky hunter, and the seal is soon dispatched. A couple of the younger men skin the animal and distribute the pieces to the assembled company as fast as needed. The testicles, being considered as the choicest titbit, are usually handed over to the hostess; the spinal cord is also rated as one of the choicest portions of the animal. During these feasts they gorge themselves to their utmost capacity, and are in good humor and hilarious. Though there may be ever so poor prospects to procure more food for the morrow, this does not deter them from gluttonously devouring the last morsel, and then go on allowance till they can get a fresh supply. I have seen them thus gorge themselves, and then lie down to sleep with a piece of seal meat by their side, which they attacked every time they awoke.

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A Spark from Menlo Park.

To the Editor of Science:

My note book is so full of observations made during a recent visit to Edison's laboratory, that I feel on looking it over as if I had struck an intellectual gold mine. The genius of Menlo Park is so exuberant, and his frankness—we may say *naiveté*—so unbounded, that we came into possession of many facts which we might almost commit a breach of confidence in exposing. I found him reserved, however, when the conversation was turned to the subject of the arc electric light, and avoiding criticism of the operations and machines of those inventors who have devoted themselves to its improvement and utilization. But he made quite merry over the opinions expressed to him by many of the sight seers who swarm to the laboratory. "Would you believe it possible," said Mr. Edison, "that in spite of the general and interesting descriptions I have seen in various publications of this and other countries, few of the visitors really know what they come to see when they ask to be shown the electric light? Many are disappointed, because we do not have a kind of inland light house with a 300 or 400 candle-power light in each pane of glass in the buildings. Others think it a 'poor show' when they examine an incandescent thread of 14 to 16 candle-power in bright sunlight."

There was one suggestion thrown off by him, while conversing about the arc electric light, which I think should not be suffered to remai nundeveloped; Mr. Edison is so devoted to 'his light' that he only has time to give an occasional thought in the other direction, and his power of concentration prevents the dispersion of his genius through a different medium. So I repeat, I do not think I am committing any breach of confidence in describing a sketch which grew up under my eye, drawn by his rapid and luminous pencil; for Edison possesses that peculiar quality of pictorial illustration which we have never seen, except in the sketches of that inventor-artist, the great Leonardo da Vinci.

"Our dynamo-machines," said he, "as we now build them, are especially constructed for the purpose of furnishing current for the incandescent lamp; but they are, of course, as easily adapted to the arc light as to other purposes. You see our lamp factory and electric railroad are run by them. A very simple addition to a machine would allow of its use in illumination where the production of reverse currents is necessary. Imagine the wire of a Gramme helix cut half way through the solenoid, the four ends joined two and two to a commutating wheel, and pairs of conductors leading to an arc light, say Jablochkoff's candles. Now, by intermittently joining the ends of the separated helices, by an appropriate arrangement on the ordinary commutator blocks, you will be able to use your main current for the small incandescent lamps, and the surplus for the arc lamp; thus supplying continuous and reverse currents from the same machine."

I hope this chance scintilla from the mind of the great inventor will be allowed to sink through the pages of my note book into your columns, without any violation of the proprieties. If it incite Mr. Edison, *en revanche*, to a development of the idea, we will bear the brunt of a, perhaps, just resentment.

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A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

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P. O. Box 3838.

SATURDAY, AUGUST 28, 1880.

THE annual session of the American Association for the Advancement of Science has been most brilliantly opened in Boston. The intellectual force now concentrated there will soon be flowing through all the channels of knowledge. Our columns next week will contain our reporter's account of the proceedings, and will be enriched by an address, in full, of the distinguished retiring President, Professor George F. Barker, whose learning and devotion to Science alone placed him in that elevated position. We have also obtained valuable and interesting papers by Professors Agassiz, Hall of Washington, and other distinguished participants, which will duly appear.

MR. PAGET HIGGS, the well-known English electrical engineer, now in Boston, has given his opinion, through the New York *Herald* (August 27), on the durability of electric motors and their actual return in work. As the general introduction of Edison's electrodyramo-machine is being anxiously looked for wherever a constant supply of cheap power is necessary, it becomes of the first importance to consumers to know how long the new engines will last. Mr. Higgs' positive statement of their length of life will no doubt confirm many small manufacturers in New York in their intention to profit by this convenient source of power, which, rumor says, will soon be generally placed at their disposal. Mr. Higgs has run some of the older and less perfect electro-motors since 1867, and finds them to-day in perfect condition. As the fruit of his own experiment and observation of the work of the most experienced European electricians, Mr. Higgs emphatically denies that there is any extraordinary loss in using them to communicate power at a distance.

WE drew attention to an educational scheme which has been recently inaugurated at the Paris Observatory for the purpose of training young astronomers. It may be interesting in this connection to know that Professor Stone, of the Cincinnati Observatory, has for a number of years been quietly but successfully pursuing a plan in almost every respect identical with that more recently inaugurated in Paris. A small number of selected graduates are admitted as students at the Observatory, pursue a systematic course of study in theoretical and practical Astronomy, and upon its successful completion receive a post-graduate degree from the authorities of the University.

The course of study carried on at the Paris Observatory is described in SCIENCE, August 14th. If there are other Observatories in the United States offering the same facilities as those initiated by Professor Stone, we shall be glad to hear from those who can give authentic information.

WE are not surprised that universal regret is expressed at the loss by the New York Fishery Commission of their annual appropriation. It appears to be acknowledged that the Commission was doing good work, and we trust their present difficulties are but temporary, and will be removed when the matter can be considered by the Legislature.

We think the Commissioners would strengthen their hands in efforts to obtain a renewal of their appropriation, if they gave some attention to the coarser kinds of fish, the supply of which appears to be practically unlimited at our very doors, and yet for unaccountable reasons is retailed at exorbitant prices, even averaging that of meat.

Fish is a natural food product for the poor of cities situated on the coast, but the dealers combine to make it an expensive luxury, by limiting the supply. We are even told that they destroy it, rather than effect sales below the prices they have arbitrarily fixed.

There appears to be little encouragement for the Legislature to grant appropriations to increase the supply of fish and lower its price, if the dealers in combination have finally the power to limit the supply and to create an artificial value.

As one of the New York Fishery Commissioners is himself one of those who are most largely interested in the sale of fish, his knowledge on the subject must be considerable, and he would certainly promote the interest of the Commission by assisting to remove the evil of which we complain. While it may be a good work to load the table of the epicure with choice fish, it should be more satisfactory to restore to the poorer classes an article of food which nature has supplied with such a bountiful hand.

ELECTRO-MOTORS.

THEIR POWER AND RETURN.

J. Hospitalier.

The transmission of force from a distance, electric ploughing, the electric railroad, etc., have made electric motors and the conditions of maximum work and maximum return, quite the order of the day. In a previous article on the available force in batteries, we have determined, for the most usual forms, the quantity of energy that could be furnished by a certain number of elements in an external circuit of proper resistance, supposing no polarization and without variation of the internal resistance.

Is this maximum of available work entirely convertible into effective work? It is not, and we will show how this maximum should be reduced when a given electric energy is to be transformed into mechanical force.

Let us suppose, for instance, in numbers, which always strike the attention more than formulas, that we have a source of electricity of 100 volts, with an internal resistance of 1 ohm. It would be easy to realize the conditions by employing an electro-dynamic machine, separately excited, or 100 very large Bunsen cups, arranged for tension in 2 parallel series of 50 each. Putting into the circuit an external resistance equal to the internal, and supposing no polarization to exist and no change in the internal resistance, we obtain as elements for the electric circulation:

E.—Electro motive force $=$ 100 volts.
r.— Internal resistance $=$ 1 ohm.
R.—Exterior resistance $=$ 1 ohm.
$(r + \mathrm{R})$—Total resistance $=$ 2 ohms.

$$\text{Q.—Quantity } \frac{E}{r+R} = \frac{100}{1+1} = 50 \text{ webers.}$$

In these conditions we know that we have in the external circuit the maximum of available work, as deduced from the formula of Joule:

$$W = 10\ Q^2R \text{ meg-ergs} \qquad (a)$$

$$\text{or } W = \frac{Q^2R}{9.81} \text{ kilogram-meters} \qquad (b)$$

In the case before us we have:

$$W = 10 \times 50^2 \times 1 = 25{,}000 \text{ meg-ergs} \quad (1)$$

What can we do with this available electric work? If we make it traverse an inert wire it will heat it. All the electric energy will be transformed into heat, and in this wire will be developed a certain number of calorics C, per second:

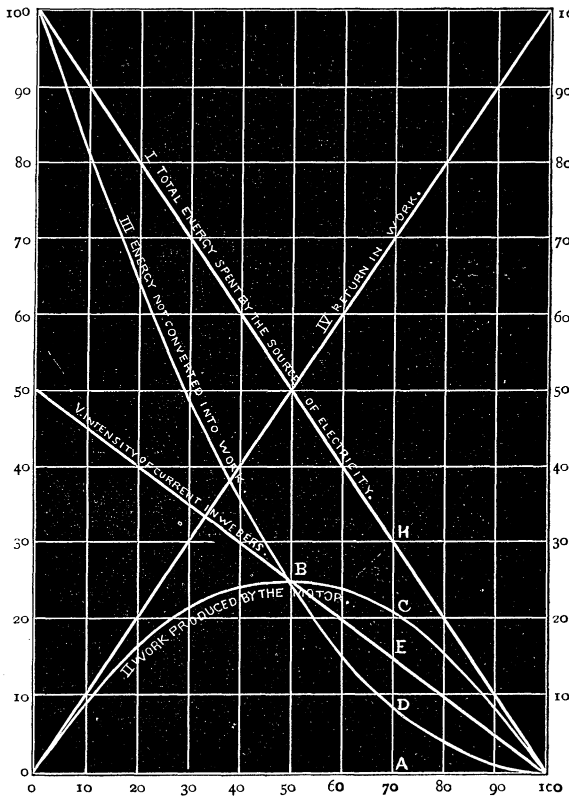
$$C = \frac{Q^2R}{9.81} \times \frac{1}{A}. \qquad (c)$$

A being the mechanical equivalent of heat 424.

Let us substitute for the inert resistance of a wire, an electro-motor of equal resistance with the wire, say 1 ohm in this particular case. Let us suppose this motor to be one of Gramme's magneto-electric machines, and that the resistance of the armature is equal to 1 ohm. If we put a break on the armature to prevent it turning under the influence of the passing current, we will not have any of the original conditions changed; the wire of the armature will be heated by the current, and a number of calorics C will be produced equal to that developed in the wire. Now let us make the armature turn under the action of the electric current. The rotary motion of this armature will develop a certain electro-motive force E′, inverse to that emanating from the source of electricity E, varying with the speed of the motor. It results in a diminution of the current, and can be expressed at each instant by the formula:

$$Q^1 = \frac{E - E'}{r + R}. \quad (d)$$

Hence the rotation of the motor diminishes the intensity of the current (and consequently the work of the motor) if a machine is employed as a source of electricity, or the consumption of zinc, if you employ a battery. The diagram shows how the different elements vary when the speed of the motor varies from zero (where the work developed is null) to a velocity such that the opposing electro-motive force E, which it develops, becomes equal to the electro-motive force of the source. It is seen that the energy expended by the source of electricity diminishes from the time the motor begins to turn (curve I.); similarly, the intensity of the current (curve V.) diminishes to zero when E and E′ become equal. Curve II. represents the work developed by the motor at different speeds. Let us suppose these speeds are proportional to the electro-motive forces—a hypothesis easily verified in a well constructed magneto-electric machine—then we see, by the diagram, an augmentation of the work produced, up to a point where the speed of the motor becomes 50. At this moment the work done is at a maximum, and represents but 50 per cent. of the work expended by the source of electricity. The energy converted into work (curve III.) is equal to what is unconverted (curve II.). If the speed augments beyond this point the work produced (curve II.) diminishes, but the return augments (curve IV.).



The work produced and the return are hence perfectly distinct things which are too often confounded. There is no impossibility in making the motor return 80 per cent. of the work expended by the source of electricity, on condition you do not make this source produce all the work which it can furnish. When, at the limit, the work produced becomes null, the return becomes equal to 1. The same conclusion is arrived at on comparing curves I. and II. It is thus seen that energy not converted into work, diminishes more rapidly than the total energy expended by the source of electricity. When the motor is at rest, the work is zero, all energy being transformed into heat. When $E' = \frac{E}{2}$, the diagram shows that the work is equal to the loss; curves II. and III. cut each other at B and the return is 50 per cent. Several consequences result from this. If you wish to obtain the *greatest* results from any given source of electricity, the electro-motor, turning at normal speed, must be so arranged as to develop a counter electro-motive force equal to the half of the original source. If the *best* results are wanted greater speed is required, by which a return in work is gained with a corresponding loss in the quantity of work produced.

Curves III. and IV. show why an electro-motor heats more when stopped than when turning at a certain speed; the intensity of the current is greater in the first case than

in the second, the electro-energy not converted into work, diminishing with increase of speed, is converted into heat in the conducting wire. The two causes are correlative.

Let us cite a case having peculiar bearing on the transmission of power at a distance by electro motors, for instance, in electric traction on railways. Suppose our motor to turn at a normal speed developing a force of 70 volts. In this condition the work produced is represented (on the diagram) by A C, the work expended on the source of electric supply by A H, and the return is 0.70. If the existing work is augmented (by putting on a brake, for instance,) it will diminish the speed of the motor; but the curve II. shows that by this very diminuition of speed the work produced by the motor augments, and a new state of equilibrium is produced very close to the first. If, on the other hand, the resisting work diminishes, the speed will augment, and the work produced will diminish. Hence we see that the work of the motor augments with the resistance, and diminishes as well with it, a most favorable condition for regulating speed and maintaining it within certain bounds not far apart. This automatic governing is not to be found in any other motor. In the latter, special apparatus has to be called into play, as in the well-known case of steam.

This statement of the theoretical conditions affecting the functions of an electro-motor supplied from a given source, shows between what limits its different elements can be made to vary. The numbers which we have given for the maximum of work in batteries, as well as those given by M. Reynier in his work on the pile, have regard only to the total available energy in the external circuit, without consideration of the manner in which this energy is ultimately used. If, as in the above hypothetical case, it is desired to transform this energy into work of an electro motor, but half of the maximum work can be obtained. If, on the other hand, it is proposed to get the greatest *sum* of work in an indefinite time, the return can be augmented and collected up to as high as 80 and 90 per cent. of the energy represented by the expenditure of zinc in the battery, but then the pile does not produce its *maximum of work*.

The influences of the external resistances remain to be examined, such as are presented in transmitting force at a distance; also the resistance of the motor itself, and the practical returns obtained in certain special cases with motors of determinate type.

We will take occasion to recur to this subject after practical experience has had the last word. It is always well, however, to recall theoretical results, which never being altogether attained in practice, have an advantage in setting exact limits to our knowledge of what can be obtained from any given source of electrical supply; and, while destroying some illusions, proving some statements, which till now, have seemed too adventurous. (*La Lumière Electrique*, Aug. 7th.)

MULTIPLE SPECTRA.*

II.

I concluded my last article under the above heading with a reference to the case of carbon, and gave the results successively arrived at by Attfield, Morren, Watts, and others; these went to show that besides the line-spectrum of carbon mapped by Angström there exists a fluted spectrum of this substance.

Now comes my own personal connection with this matter.

In the year 1871,[1] I communicated to the Royal Society a paper in which the conclusion was drawn that the vapor of carbon was present in the solar atmosphere.

This conclusion was founded upon the reversal in the solar spectrum of a set of flutings in the ultra-violet.[2] The conclusion that these flutings were due to the vapor of carbon, and not to any compound of carbon, was founded upon experiments similar to those employed in the researches of Attfield and Watts, who showed that the other almost exactly similar sets of flutings in the visible part of the spectrum were seen when several different compounds of carbon were exposed to the action of heat and electricity. In my photographs the ultra violet flutings appeared under conditions in which carbon was the only constant, and it seemed therefore reasonable to assume that the flutings were due to carbon itself, and not to any compound of carbon, and this not alone from the previous work done in the special case of carbon, but from that which had shown that the fluted spectra of sulphur, nitrogen, and so forth, were really due to these "elementary" substances.

Professors Liveing and Dewar have recently on several occasions called this result in question. Professor Dewar, in a paper received by the Royal Society on January 8, 1880, writes as follows:

"The almost impossible problem of eliminating hydrogen from masses of carbon, such as can be employed in experiments of this kind, prove conclusively that the inference drawn by Mr. Lockyer, as to the elementary character of the so-called carbon spectrum from an examination of the arc in dry chlorine, cannot be regarded as satisfactory, *seeing that undoubtedly hydrogen was present in the carbon* used as the poles.

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It will be seen that on January 8 Mr. Dewar alone attributed the flutings to a hydrocarbon, while on February 2 Mr. Dewar, associated with Mr. Liveing, attributed them to a nitrocarbon.

In fact in the latter paper Messrs. Liveing and Dewar published experiments on the spectra of various carbon compounds, and from their observations they have drawn the conclusion that the set of flutings which I have shown to be reversed in the solar spectrum is really due to *cyanogen*, and that certain other sets of flutings shown by Attfield and Watts to be due to carbon are really due to hydrocarbon.

As Messrs. Liveing and Dewar do not controvert the very definite conclusions arrived at by Attfield, Morren, Watts, and others, I can only presume that they took for granted that all the experimental work performed by these men of science was tainted by the presence of impurities, and that it was impossible to avoid them. I therefore thought it desirable to go over the ground again, modifying the experimental method so as to demonstrate the absence of impurities. Indeed I have started upon a research which will require some time to complete. Still, in the meantime, I have submitted to the notice of the Royal Society some results which I have obtained, which I think settle the whole question, and it is the more important to settle it as Messrs. Liveing and Dewar have already based upon their conclusions theoretical views which appear to me likely to mislead, and which I consider to have long been shown to be erroneous. To these results I shall now refer in this place.

The tube with which I have experimented is shown in Fig. 1: A and B are platinum wires for passing the spark inside the tube; E is a small tube into which carbon tetrachloride was introduced; it was drawn out to a long narrow orifice to prevent the rapid evaporation of the liquid during the exhaustion of the tube. The tube was bent upwards and a bulb blown at C in order that the spark might be examined with the tube end-on, as its found that after the spark has passed for some time a deposit is formed on the sides of the bulb immediately surrounding the platinums, thus obstructing the light. After a vacuum had been obtained the tube was allowed to remain on the Sprengel pump, to which it was attached by a mercury joint for the purpose of obtaining a vacuum for a long time, in order that the last traces of air and moisture might be expelled by the slow evaporation of the liquid.

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The carbon tetrachloride was prepared by Dr. Hodgkinson, who very kindly supplied me with sufficient for my experiments.

On passing the spark without the jar in this tube, the spectrum observed consists of those sets of flutings which, according to Messrs. Liveing and Dewar, are due to hydrocarbon, and the set of flutings which is reversed in the sun, and ascribed by Messrs. Liveing and Dewar to cyanogen, also appears in a photograph of the violet end of the spectrum, Fig. 2. On connecting a Leyden jar with the coil and then passing the spark the flutings almost entirely vanish and the line spectra of chlorine and carbon take the place of the flutings without either a line of hydrogen or a line of nitrogen being visible.

As a long experience has taught me that these tubes often leak slightly at the platinums after they are detached from the pump, so that the evidence of such a *pièce justificatif* is only good for a short time, I took the occasion afforded by

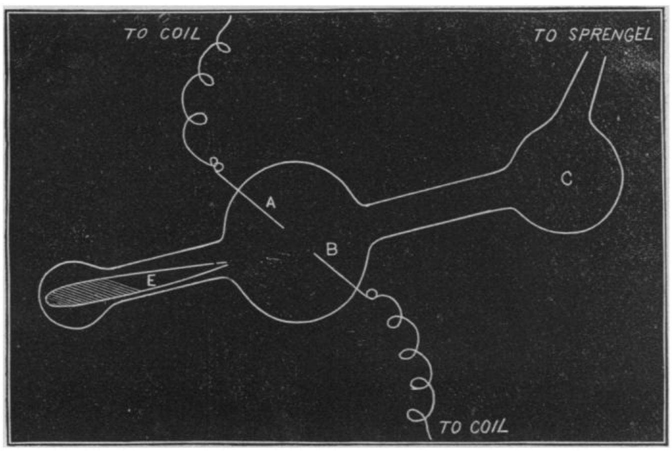

FIG. 1.

a visit of Dr. Schuster to my laboratory while the experiments wese being made to get my observations confirmed. He has been good enough to write me the following letter and to allow me to give it here:—

"*March* 21.

"MY DEAR LOCKYER.—The following is an account of the experiment which I saw performed in your laboratory on Monday, March 15:

"A tube containing carbon-tetrachloride was attached to the Sprengel pump. As exhaustion proceeded the air was gradually displaced by the vapor of the tetrachloride. The electrodes were a few millimetres apart. If the spark was

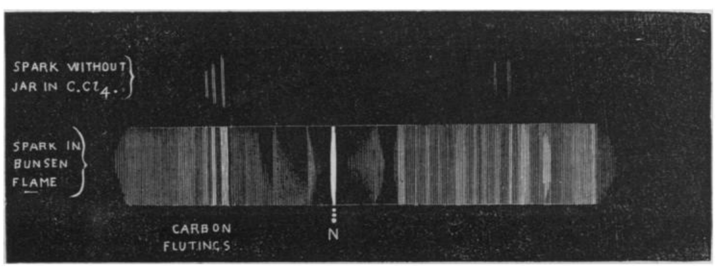

FIG. 2.

taken without a condenser in the vapour the well-known carbon bands first observed by Swan in the spectrum of a candle were seen with great brilliancy; I also saw the blue band which you said was identical in position with one of the blue bands seen in the flame of cyanogen or in the spectrum of the electric arc. When the condenser and air-break were introduced this spectrum gave way to a line spectrum in which I could recognize the lines of chlorine. *The lines of nitrogen were absent, not a trace of the principal double line in the green being seen.* The hydrogen line Hα(C) was faintly visible when I first observed the spectrum, but it got gradually weaker and *finally disappeared altogether. When this line was no longer visible the condenser was taken out of circuit again, and the same carbon bands were seen as before.* These bands, therefore, show themselves with great brilliancy when a strong and powerful spark does not reveal the presence either of hydrogen or nitrogen. (Signed) ARTHUR SCHUSTER."

"March 21, 1880."

This result, which entirely endorses the work of Attfield and Watts, has been controlled by many other experiments. I have also repeated Morren's experiment and confirm it and I have also found that the undoubted spectrum of cyanogen is visible neither in the electric arc nor in the surrounding flame.

Hence then in the case of carbon, as in the prior cases of hydrogen, nitrogen and the like, those who hold that the flutings are due to impurities must, it would seem, abandon their position; for the flutings are undoubtedly produced by carbon vapor. Nor is this all; the suggestion that the various difficulties which have always been acknowledged to attend observations of this substance may in all probability be due to the fact that the sets of carbon flutings represent different molecular groupings of carbon, in addition to that or those which give us the line spectrum, and that the tension of the current used now brings one set of flutings into prominence and now another, seems also justified by the facts. This suggests the view that a body may have a fluted spectrum of compound origin as well as a line spectrum.

This conclusion is greatly strengthened by the preliminary discussion of a considerable number of photographs of the spectra of various carbon compounds.

A general comparison of the photographs first enables us to isolate the lines in the blue and ultra-violet portions of the spectrum (wave lengths 4300–3800) of the substance associated with the carbon in each case.

In this manner the lines seen in the photographs of the

spectra of CCl_4, $C_{10}H_8$, CN, CHI_3, CS_2, CO_2, CO, &c., have been mapped, and both the common and special lines and flutings thus determined.

The phenomena seen with more or less constancy are a blue line, with a wave-length of 4266 ; a set of blue flutings, extending from 4215 to 4151 ; and another set of ultra-violet flutings, which extend from 3885 to 3843 (all approximate numbers).

In a photograph of the spectrum of the electric arc (with a weak battery) between carbon poles in an atmosphere of chlorine, the blue flutings alone are visible, whilst, when the *spark* is similarly photographed, the ultra-violet flutings and the blue line (4266) are also visible, whilst the blue flutings become fainter.

From this we may assume, in accordance with the working hypothesis of a series of different temperature furnaces, as set forth in the paper of December, 1878 (see Fig. 3), that the different flutings and the line correspond to different temperature spectra, the blue flutings to the lowest and the blue line to the highest temperature, whilst the ultra-violet flutings occupy an intermediate position.

According to this working hypothesis there should be the spectrum which contains the blue line alone and that which contains the blue fluting alone (Fig. 4). In comparing the spectra of carbon under different conditions, I find this to be true. *The blue line never appears in conjunction with the blue flutings, unless the ultra-violet flutings are also present.* In other words, the highest and the lowest hypothetical temperature spectra are never visible together without the spectrum of the intermediate hypothetical temperature.

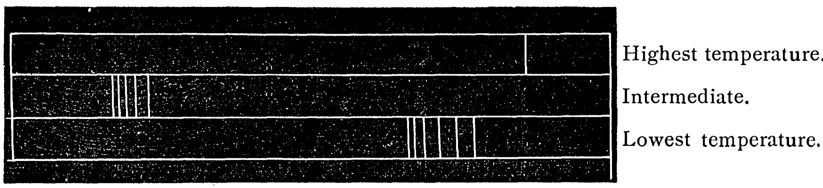

FIG. 3—Action of three different temperatures on a hypothetical substance, assuming three stage of complete dissociation.

But this is not all. By placing the spectra of the substances at different heat-levels, so to speak, I was enabled to construct a map, which not only indicates the mere presence or absence of the lines and their relative intensities, but shows a perfect gradation between the spectrum which contains the line alone and that which contains the blue flutings alone (Fig. 5). I would point out that there is nothing theoretical in this map. All the horizons depicted are copied from photographs of carbon under the conditions indicated, and theory has merely enabled me to arrange them *in order*.

This map I submit, therefore, bears out the hypothesis of differences of temperature indicated above, for it is seen a series of horizons forming a perfect gradation between that, while the blue line gradually thins out, the ultra-violet flutings appear first and grow in intensity. As these increase the blue flutings become visible, and further, as the

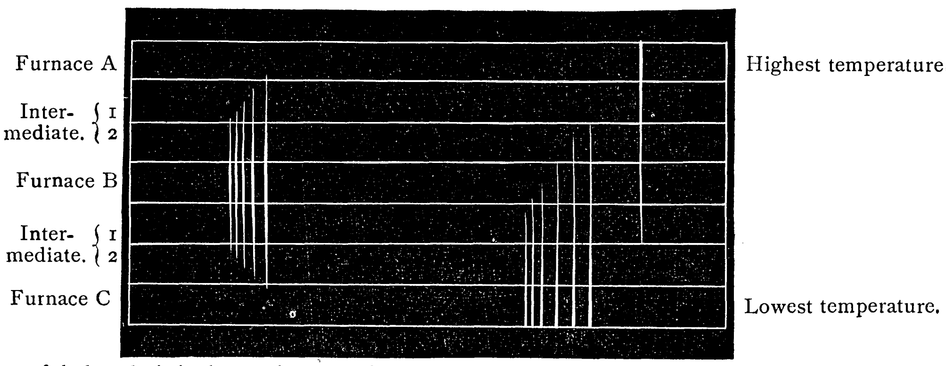

FIG. 4—Spectra of the hypothetical substance, in intermediate furnaces, assuming that the vapours are not completely dissociated.

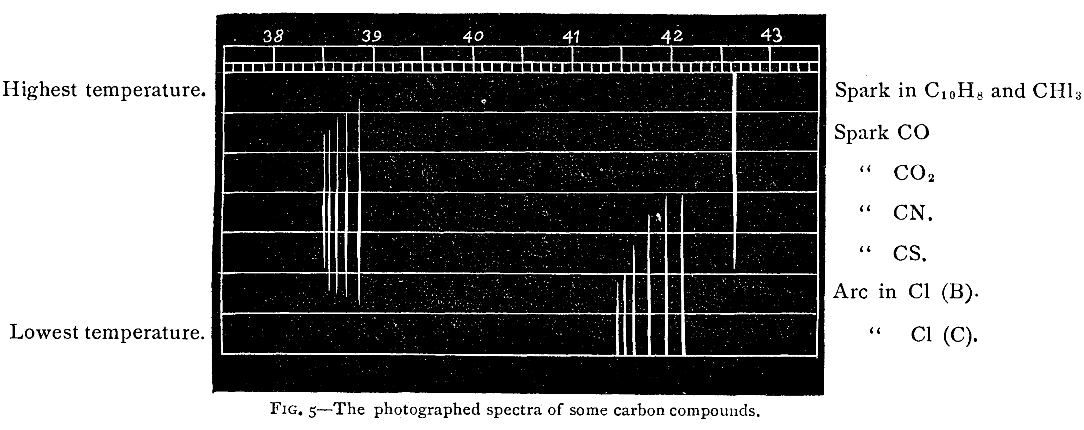

FIG. 5—The photographed spectra of some carbon compounds.

latter augments and the line disappears, the ultra-violet flutings gradually die out altogether.

It is philosophical to infer from these observations that not only are the line and flutings in question produced by carbon, but that the blue line (4266), since it is visible at the highest temperature, corresponds to the most simple molecular groupings we have reached in the experiments, and the flutings to others more complex.

The result to which attention is most to be directed in this place is that touching the two sets of flutings, and should future research justify the double conclusion (1) that these flutings are truly due to carbon, a result I accept, though it is denied by Angström and Thalèn; and (2) that the different flutings really represent the vibrations of different molecular groupings; a great step, and one in the direction of simplification, will have been gained.

Indeed it is much to be hoped that this ground will be at once worked over again by men of science who are both honest and competent: that the truth is sure to gain by such work is a truism.

I have so often taken occasion to refer with admiration to the work of Angström and Thalèn that I shall not be misunderstood when I say that their conclusions, to which such prominence is given, and on which such great stress is laid by Messrs. Liveing and Dewar, rest more upon theory and analogy than upon experiment.

Their work, undertaken at a time when the existence of so-called "double spectra" was not established upon the firm basis that it has now, and when there was no idea that the spectrum recorded for us the results of successive dissociations, gave, as I have previously taken occasion to state, the benefit of the doubt in favor of flutings being due to compounds, and it was thought less improbable that cyanogen or acetylene should have two spectra than that carbon or hydrogen should possess them.

Indeed, later researches have thrown doubt upon the view that the fluted spectra of aluminium and magnesium are entirely due to the oxides of those metals instead of to the metals themselves—and this is the very basis of the analogy which Angström and Thalèn employed.

The importance of the observations to which I have referred is all the greater because of the general conclusions touching other spectra which may be drawn from them. Thus from what I have shown it will be clear that if my view is correct, the conclusions drawn[1] by Messrs. Liveing and Dewar from the assumed hydrogen-carbon bands touching both the spectrum of magnesium and the spectra of comets, are entirely invalid. These conclusions are best given in their own words:—

"The similarity in the character of the magnesium-hydrogen spectrum, which we have described, to the green bands of the hydrocarbons is very striking. We have similar bright maxima of light, succeeded by long drawn-out series of fine lines, decreasing in intensity towards the more refrangible side. This peculiarity, common to both, impels the belief that it is a consequence of a similarity of constitution in the two cases, and that magnesium forms with hydrogen a compound analogous to acetylene. In this connection the very simple relation (2 : 1) between the atomic weights of magnesium and carbon is worthy of note, as well as the power which magnesium has, in common with carbon as it now appears, of combining directly with nitrogen. We may with some reason expect to find a magnesium-nitrogen spectrum. . . .

"The interest attaching to the question of the constitution of comets, especially since the discovery by Huggins that the spectra of various comets are all identical with the hydrocarbon spectrum, naturally leads to some speculation in connection with conclusions to which our experiments point. Provided we admit that materials of the comet contain ready-formed hydrocarbons, and that oxidation may take place, then the acetylene spectrum might be produced at comparatively low temperatures without any trace of the cyanogen spectrum or of metallic lines. If, on the other hand, we assume only the presence of uncombined carbon and hydrogen, we know that the acetylene spectrum can only be produced at a very high temperature, and if nitrogen were also present that we should have the cyanogen spectrum as well. Either, then, the first supposition is the true one, not disproving the presence of nitrogen, or else the atmosphere which the comet meets is hydrogen only, and contains no nitrogen."

[1] Paper read February 12, 1880.

The importance of the question here treated of comes out very well from these two extracts. We find the same spectral phenomenon at once called into court, and very properly called in, both to suggest the existence of chemical substances of which the chemist has never dreamt, and to explain the chemical nature of a large group of celestial bodies.[1]

There is little doubt that when a complete consensus of opinion is arrived at among the workers, other suggestions more far reaching still will be derived from the prosecution of these inquiries. For the present, however, the chief point to bear in point is that both in line-spectra and in fluted spectra we have indications which I think favor the view that in each case the origin is compound rather than simple.—*Nature*. J. NORMAN LOCKYER.

OBAN, *July* 20.

[1] With special reference to this last question, that of cometary spectra, one of acknowledged difficulty, I may perhaps be permitted to add here by way of note that the view I put forward some years ago touching the relation to this spectrum to that of the nebulæ has been lately strengthened by the observation that at a low temperature one of the brightest lines in the spectrum of iron is that coincident with the chief line in the nebula-spectrum.

PHYSICAL NOTES.

FROM the above article we see that as far back as 1878, Mr. Lockyer communicated to the Royal Society a paper in which the conclusion was drawn that vapor of carbon was present in the solar atmosphere. This inference was founded upon experiments similar to those of Attfield and Watts, who showed that flutings are always present in different compounds of carbon exposed to the action of heat and electricity. This observation of Lockyer has been called in question by Liveling and Dewar, as they have found it an almost impossible problem to eliminate hydrogen from masses of carbon. This latter view has been long held by Edison, who, in a great number of experiments, some of which were participated in by Prof. Young, has found at the enormous heat developed by igniting a fine carbon thread $\frac{8}{1000}$ of an inch diameter, of high resistance, in air vacuum, until a light of 80 candles is reached, that only a carbon spectrum is given, until just a few seconds before the rupture of the loop, *when a sharply defined hydrogen spectrum is observed.* On the other hand, in an observation of the purified spectrum of carbon tetrachloride, Mr. Lockyer (*Nature*, August 5th) found only carbon appeared at high temperatures. It is an excellent index of the spirit of unbiased investigation in the author of (*Nature*, December, 1878) The Hypothesis that the so-called Elements are Compound Bodies, and still later, of the Universal Hydrogen Hypothesis, to learn from Mr. Lockyer that, both in line and fluted spectra, he thinks we have indications which favor the view that in each case the origin is compound rather than simple.

IN a communication from William Huggins, F.R.S., received June 16th, 1880, and published in the *American Journal of Science* for August, are embodied some observations on the nature of the spectrum of water, which may give rise to a question of priority. It appears that Dr. Huggins made a photograph of the flame of hydrogen burning in air, December 27, 1879, but did not publish the fact.

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latter augments and the line disappears, the ultra-violet flutings gradually die out altogether.

It is philosophical to infer from these observations that not only are the line and flutings in question produced by carbon, but that the blue line (4266), since it is visible at the highest temperature, corresponds to the most simple molecular groupings we have reached in the experiments, and the flutings to others more complex.

The result to which attention is most to be directed in this place is that touching the two sets of flutings, and should future research justify the double conclusion (1) that these flutings are truly due to carbon, a result I accept, though it is denied by Angström and Thalèn; and (2) that the different flutings really represent the vibrations of different molecular groupings; a great step, and one in the direction of simplification, will have been gained.

Indeed it is much to be hoped that this ground will be at once worked over again by men of science who are both honest and competent: that the truth is sure to gain by such work is a truism.

I have so often taken occasion to refer with admiration to the work of Angström and Thalèn that I shall not be misunderstood when I say that their conclusions, to which such prominence is given, and on which such great stress is laid by Messrs. Liveing and Dewar, rest more upon theory and analogy than upon experiment.

Their work, undertaken at a time when the existence of so-called "double spectra" was not established upon the firm basis that it has now, and when there was no idea that the spectrum recorded for us the results of successive dissociations, gave, as I have previously taken occasion to state, the benefit of the doubt in favor of flutings being due to compounds, and it was thought less improbable that cyanogen or acetylene should have two spectra than that carbon or hydrogen should possess them.

Indeed, later researches have thrown doubt upon the view that the fluted spectra of aluminium and magnesium are entirely due to the oxides of those metals instead of to the metals themselves—and this is the very basis of the analogy which Angström and Thalèn employed.

The importance of the observations to which I have referred is all the greater because of the general conclusions touching other spectra which may be drawn from them. Thus from what I have shown it will be clear that if my view is correct, the conclusions drawn[1] by Messrs. Liveing and Dewar from the assumed hydrogen-carbon bands touching both the spectrum of magnesium and the spectra of comets, are entirely invalid. These conclusions are best given in their own words:—

"The similarity in the character of the magnesium-hydrogen spectrum, which we have described, to the green bands of the hydrocarbons is very striking. We have similar bright maxima of light, succeeded by long drawn-out series of fine lines, decreasing in intensity towards the more refrangible side. This peculiarity, common to both, impels the belief that it is a consequence of a similarity of constitution in the two cases, and that magnesium forms with hydrogen a compound analogous to acetylene. In this connection the very simple relation (2 : 1) between the atomic weights of magnesium and carbon is worthy of note, as well as the power which magnesium has, in common with carbon as it now appears, of combining directly with nitrogen. We may with some reason expect to find a magnesium-nitrogen spectrum. . . .

"The interest attaching to the question of the constitution of comets, especially since the discovery by Huggins that the spectra of various comets are all identical with the hydrocarbon spectrum, naturally leads to some speculation in connection with conclusions to which our experiments point. Provided we admit that materials of the comet contain ready-formed hydrocarbons, and that oxidation may take place, then the acetylene spectrum might be produced at comparatively low temperatures without any trace of the cyanogen spectrum or of metallic lines. If, on the other hand, we assume only the presence of uncombined carbon and hydrogen, we know that the acetylene spectrum can only be produced at a very high temperature, and if nitrogen were also present that we should have the cyanogen spectrum as well. Either, then, the first supposition is the true one, not disproving the presence of nitrogen, or else the atmosphere which the comet meets is hydrogen only, and contains no nitrogen."

[1] Paper read February 12, 1880.

The importance of the question here treated of comes out very well from these two extracts. We find the same spectral phenomenon at once called into court, and very properly called in, both to suggest the existence of chemical substances of which the chemist has never dreamt, and to explain the chemical nature of a large group of celestial bodies.[1]

There is little doubt that when a complete consensus of opinion is arrived at among the workers, other suggestions more far reaching still will be derived from the prosecution of these inquiries. For the present, however, the chief point to bear in point is that both in line-spectra and in fluted spectra we have indications which I think favor the view that in each case the origin is compound rather than simple.—*Nature*. J. NORMAN LOCKYER.

OBAN, *July* 20.

[1] With special reference to this last question, that of cometary spectra, one of acknowledged difficulty, I may perhaps be permitted to add here by way of note that the view I put forward some years ago touching the relation to this spectrum to that of the nebulæ has been lately strengthened by the observation that at a low temperature one of the brightest lines in the spectrum of iron is that coincident with the chief line in the nebula-spectrum.

PHYSICAL NOTES.

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ter bearing date 30th January, 1880, spoke of "a novel and interesting result," referring, probably, to the above-mentioned photograph. Since then, Dr. Huggins has taken a large number of photographs of the spectra of different flames, but only presents one (that of hydrogen) to the Royal Society. We regret this, both because of the loss to our general stock of science, in this unnecessary detention of the spectrum of carbon and its compounds, and because of the imminent probability of a repetition of these disagreeable questions of priority, as, on this side of the water (to the writer's knowledge), this particular subject is being eagerly studied under unique conditions.

The experiment of Dr. Huggins consists of first burning hydrogen *per se* in atmospheric oxygen, and then a mixture of oxygen with hydrogen in air. He finds the two spectra identical. For purposes of comparison, he very ingeniously photographs them on the same plate, in rapid succession, using the upper half of his spectroscope slit for the first, and the lower half for the second impression. As all the lines of both spectra fit each other exactly, without excess, it is evident that either represents the spectrum of water. The article referred to contains a partial spectrum, giving the characte ristic lines of wate.

PROF. J. TROWBRIDGE has recently studied the earth as a conductor of electricity and details some interesting experiments, and advances some bold speculations and prophecies in the *American Journal of Science* for August. In all the telephone circuits between Boston and Cambridge for a distance of about four miles, the ticking of the Observatory clock could be heard when transmitting time signals. This was attributed to the proximity of the telephone circuit wires to the time wires of the Observatory. Mathematical considerations, however, (Maxwell's Electricity and Magnetism, Vol. II., p. 209), will convince one that with telephones of the resistance usually employed, no inductive effect will be perceived between wires which run parallel to each other a foot apart for the distance of thirty or forty feet, even if ten-quart Bunsen cells be used. The transmission of these time signals is evidently not due to induction, but to tapping the earth, so to speak, at points which are not in the same potential. Running a wire five or six hundred feet long to ground at both ends, and putting a telephone in circuit, the ticking was distinctly heard when an exploration was made in an open field an eighth of a mile from the Observatory; yet the same wire, under similar conditions, gave no sound when one mile away from the central line between the Observatory and the Boston office. With the boldness of a Gallileo, Professor Trowbridge deduces thence the theoretical possibility of telegraphing across the Atlantic without a cable. He says: "Powerful dynamo-electric machines could be placed at some point in Nova Scotia, having one end of their circuit grounded near them and the other end grounded in Florida, the conducting wire consisting of a wire of great conductivity and carefully insulated from the earth, except at the two grounds. By exploring the coast of France, two points on two surface lines not at the same potential could be found; and by means of a telephone of low resistance, the Morse signals sent from Nova Scotia to Florida could be heard in France. Theoretically this is possible, but practically, with the light of our present knowledge, the expenditure of energy on the dynamo-electric engine would seem to be enormous."

A VERY curious observation has been made by M. J. Janssen of a remarkable inversion in a photographic image by exposure during different times. It passed from negative to positive with an intermediary neutral, invisible period. After a first exposure of $\frac{1}{1300}$ of a second a negative can be developed, a little longer exposure would dull the sharpness of the image; then there soon arrives a point where the negative disappears entirely. By a still longer exposure a new phase occurs, a positive image starts out from the plate, with lights and shadows just the reverse of the first and as sharply defined. By allowing further action of the light a second neutral condition occurs. M. Janssen does not say by what state this is followed.—*Moniteur Sci.*

M. SCHEURER-KESTNER in a note to the Académie des Sciences, qualifies a previous statement that sulphuric acid attacks platinum, by new experiments. Absolutely pure sulphuric acid does not attack platinum, but if there be ever so small a content of nitrous acid, a very appreciable quantity of the vessel is dissolved, $\frac{1}{10000}$ being enough for the purpose. In one of his experiments, on 60 grams of sulphuric acid, two milligrams of platinum were dissolved. This fact should be verified by manufacturers of concentrated sulphuric acid.

Mr. Albert Levy finds considerable variation in the ammoniacal contents of rain waters collected in the different quarters of Paris, but the annual means are identical. The per centages diminish from one month to the next, in passing from the cold to the hot season. The minimum at all stations was for the month of July, when there was present .93 of a milligram of nitrogen, against 1.35 in January. The potable waters of Paris are affected in exactly the same way. The reverse, however, is the case with the ammonia of the air which is most abundant in the hot season.—*Moniteur Scientifique*, Aug.

THE organisms described by Pasteur as the origin of epidemics and contagious disease, are so minute and few compared with the multiplying swarms of bacteria, etc., pervading all generating solutions, that it becomes necessary to provide a means of eliminating the masses of infusoria from solutions to be studied under the microscope. These microzoa haunt even the clearest drinking water at times, and it becomes highly important to easily determine their presence. M. Certes (Proceedings Acad. des Sciences), suggests the use of osmic acid as a sure means of killing them without destroying their tissues. He dips a glass rod into the solution to be examined and then into a 1½ per cent. solution of the acid; washing this in a narrow test tube of distilled water, it is easy to collect what is necessary for examination. There are certain precautions to be taken as to cleanliness and time of immersion. By the use of a mixture of Paris violet in diluted glycerine, he finds it possible by uniform difference of tint, to easily distinguish cellulose, amylaceous matter and the vibrating cilia.

M. DE LESSEPS, as an argument against the quarantine system, read a letter to the French Academy of Science, from the engineer in charge of the preparatory work of the interoceanic canal, informing him that a number of persons had disembarked at the isthmus while sick of yellow fever, without having propagated the disease among the workmen. Following this communication of M. De Lesseps, M. Bouley said he could not allow the inference from such remarks to pass unchallenged. Admitting that what M. De Lesseps said was true, that quarantines are a constant inconvenience to commercial and maritime relations, yet this injury is in the highest degree compensated for by the guarantees given to the public health. Since the international sanitary police has been watching over Egypt, and preserving it from the invasion of cholera by strict quarantine, this disease had come to be less feared in Europe. It is by quarantine alone we shelter ourselves from those diseases which vessels so easily carry with them, particularly the yellow fever to which M. De Lesseps refers. The atmospheric conditions which he says render quarantines nugatory, cannot contribute to the propagation of epidemics, unless those who are attacked are allowed to land from the vessels which contain the germs. But these germs are not intangible exhalations, subtile vapors, effluvia which have a property of fatal expansion, against which we can do nothing. Quite the contrary is true. Thanks to the researches of experimental science, the principle of contagion is no longer unknown; it has taken body and can be studied and followed in its manifestations. But even before this accession to our knowledge, practice, inspired by observation, had proved that strict surveillance of men and things coming from suspected countries would prevent the spreading of the germs. This is the province of the quarantine and by it alone can it be done. It is, then, necessary to maintain it in spite of the convenience to commercial and maritime relations.

OTTO A. MOSES.